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GEOLOGY.—*On the terms aphrolith and dermolith.* T. A. JAGGAR, JR., Hawaiian Volcano Observatory. (Communicated by Arthur L. Day.)

Dutton[1] in describing Hawaiian volcanoes made use of the terms aa and pahoehoe, and these words of Hawaiian origin have been somewhat adopted by American geologists. Some English geologists object to them strongly and continental usage is various in the matter. Aa lava of the Hawaiians is characterized by a peculiar mechanism of solidification of olivine basalt, whereby a magma surcharged with gas so froths or foams in its surface layer that on solidification this layer shrivels and separates into discrete scoriaceous units, in some places light and pumiceous, but elsewhere heavy and slaggy. The extreme pumiceous type is the limu, or "thread-lace scoria" of Dana. The lava flow beneath solidifies as a coarsely vesicular body grading downward into crystalline rock which has no known petrographical peculiarity different from any other type of flow. The boundary surface at the top of the continuous sheet and below the discontinuous layer of scoriae is revealed at many places on the shoreline of Hawaii, where the ocean has swept the loose material away. In these places the surface revealed is rugose and in detail much like the scoriaceous fragments, but it sometimes exhibits coarse ropy forms and rough festoons

[1] DUTTON, C. E. *Hawaiian Volcanoes.* 4th Ann. Rep. U. S. Geol. Surv., 1882-83.

in plan. The distinctive feature of typical aa, however, is the shrinkage of the surface into units from a few centimetres to several metres in diameter, each unit being complete in itself and commonly free from broken surfaces. Such lava is spoken of in text books which have not adopted the Hawaiian word, as fragmentary lava, block lava, or scoriaceous lava, and in the southwestern states the Mexican word "malapai" or "malapis" is used for the vast fields of aa which there occur. In the Cordilleran belt and elsewhere great bodies of ancient aa are called pyroclastics, breccias, and agglomerates indiscriminately, along with explosion products that are of totally different origin. Aa makes a pyroclastic where igneous matrix is apt to be identical with the contained fragments, and the fragments are of irregular lumpy outline.

The pahoehoe lava of the Hawaiians, on the other hand, is distinguished by smooth surfaces which result from the formation of a glassy membrane or skin. The mode of congelation of olivine basalt magma advancing as a pahoehoe flow is very strikingly different from that of an aa flow. The surface of the lava lake of Kilauea in all stages of its crusting is typically pahoehoe. As the heat is radiated from the surface, solidification forms a dermal layer made of filaments of glass remnant from expanding bubbles filled with gas, and these show no tendency to burst explosively, but incessantly crowd into the interstices of the stretching and rending layer of filaments of previous formation. The result is a skin not usually gas tight and exhibiting extraordinary diversity of texture. Solidification proceeds from without inwards, the skin reaches various thicknesses and then folds, and the folding distributes itself somewhat like the moraines of a glacier with festoons drawn downstream farthest along the middle line and retarded at the two sides. At the sides there are lengthwise curtains produced by the longitudinal drag between resistant cooling margins and the festooned stream in the middle. At the front, toes of the molten fluid push out from under the skirts of the flow in peculiar tuberous bodies, which tend to congeal on all sides and may even lift their tips vertically and solidify. They ap-

pear to be blown by expanding gas, for the axis is almost tubular in its coarser vesicularity. The spasmodic renewed advance of a pahoehoe flow is frequently characterized by pushing out from under a skirt of crust, in contrast to the aa flow which appears to solidify from below upward and to advance by overriding its already solidified lobes. Pahoehoe lava is variously mentioned in the text books as fluent, smooth, ropy, or corded lava.

Visually the two types of lava, in mobility and temperature, appear much alike while incandescent and flowing rapidly as golden streams. With the beginnings of solidification, however, the difference is pronounced, pahoehoe forming skins and aa forming knobs and cinders like a bed of hot coals. The cooling front of an aa flow is full of flames through the apertures of the blocks. The front of a pahoehoe flow shows no flames detectable by eye. The aa flow at its front is in a brittle semi-solid condition, slabs rifting away and falling forward from a mass beneath that resembles hot iron. Pahoehoe, on the other hand, bellies its crust forward, escapes as a viscous pudding and this in turn skins over, the whole movement resembling the advance of candy or tar.

The explanation of the physico-chemical difference between the two types remains to be discovered, as chemical analysis in the same region has as yet revealed no distinction. It seems probable that the quantity of confined gas, in solution or in bubble form, for each unit of volume of melt, controls the mode of freezing. Possibly the gases are nearer equilibrium in pahoehoe than in aa. The heat equation plays an important role, and this involves reaction between the gases as well as their oxidation in air. Gas expansion may be more rapid in aa and so induce internal solidification. Furthermore, there are enormous differences in the state of oxidation of the iron at the moment of cooling, and as yet we know nothing of the progress of crystallization in the field. With so many variables there is no cause for wonder that the distinction is as yet unexplained.

Between pahoehoe and aa flows in Hawaii there are unquestionably gradations of many appearances and perhaps of many

kinds, but this the writer doubts. By this is meant that the gradations may be mixtures of two substances rather than stages of one substance. This is only a suggestion in the present condition of our knowledge, based on the discovery, reported in a previous note, that the lake bottom or bench magma of Kilauea was found solidified as aa, whereas the lake magma solidifies as pahoehoe.

In view of the diversity of usage cited above, and of the confusion which results when pahoehoe crusts break up and make a "fragmentary" or "block" lava not at all of aa character, or when aa flows are revealed scoured bare and show "ropy" or "corded" aspect without being in the least of pahoehoe origin, I believe that volcanology needs a terminology scientifically descriptive of the two groups of effusive magmatic bodies, and capable of adaptation as adjectives, adverbs, or nouns in any modern language.

Accordingly I propose for pahoehoe the term DERMOLITH (Greek, skin + stone), to cover all those forms of effusive lava which manifest subaerially a surface skin or crust, capable of wrinkling or folding, as the chief distinguishing character.

For example, dermolithic basalts are common in Hawaii, Samoa, Iceland, Reunion, and on the Snake River plains, but dermolithic flow has not been identified at Lassen's Peak. At Mauna Loa near the vents of 1916, the lava flowed dermolithically in a coarse way, but exhibited a much rougher texture than the characteristic dermolith of Halemaumau. On the floor of Kilauea crater great fields of dermolith extend far and wide.

I propose for aa the term APHROLITH (Greek, foam + stone), to distinguish those effusive lavas or crater bodies which tend on solidification to subdivide surficially into complete irregular lumpy vesicular units, the crust surfaces of the continuous mass having protuberances of similar rugged character.

For example, aphrolithic lava flow has been dominant on the southern flank of Mauna Loa since 1868, the flow of that year being partly dermolithic. It would appear also, from the observations of 1917, that aphrolith occupies the bottom of the Kilauea lava lake and that hence the solidification of the interior

of the bench magma must proceed aphrolithically. Supposing the cumulo-domes of Bogoslof and Pelée to be essentially aphrolith, it is a matter of great interest to determine whether any dermolithic flow, however small, took place in those magmas. At Soufriere in St. Vincent, and also on Bandaisan, there are aphrolithic blocks of fresh rock quite distinct from the angular fragments, and apparently representative of the subterranean magmas which produced the explosions.

If these terms are adopted, it is hoped that they will not be changed in English to the *-lite* termination, as Harker[2] insisted on doing for "batholite," etc. The *-lite* syllable is needlessly confusing, on account of the innumerable rock and mineral names so ending. Harker's contention is that "-lith" is continental and not English, but this is disproved by such a word as *monolith*, which is accepted English of Greek derivation.

GENETICS.—*Observed changes in hereditary characters in relation to evolution.*[1] H. S. JENNINGS, Johns Hopkins University.

The problem of the method of evolution is one which the biologist finds it impossible to leave alone, although the longer he works at it, the farther its solution fades into the distance. The central point in the problem is the appearance, nature, and origin of the heritable variations that arise in organisms; the changes that occur in the hereditary constitution. I have for a long time been studying the appearance of heritable variations in certain lower organisms. Having satisfied myself as to the nature of the variations that arise in the creatures that I have studied, I have looked about to see what other workers have found; and to determine whether any unified picture of the matter can be made. Can we bring these facts which experimental work has brought out into relation with the method of evolution? Can we say that they exclude any particular theory?

[2] HARKER, A. Natural History of the Igneous Rocks.

[1] A lecture delivered before the Washington Academy of Sciences, March 15, 1917.

Can we say that they leave certain views admissible? Can we go farther and say that they make certain views probable? I shall hardly be so bold as even to ask whether they establish any particular views, though even that has been at times affirmed.

These questions have, of course, been raised thousands of times; it is only because knowledge does advance, because experimental work has been enormously multiplied of late, that there is reason to bring them up anew. I am going to try to put before you the present situation as it appears to me.

What we may call the first phase of the modern experimental study of variation is that which culminated in the establishment of the fact that most of the heritable differences observed between closely related organisms—between the members of a given species, for example—are not *variations* in the sense of alterations: are not active *changes* in constitution, but are permanent diversities; they are static, not dynamic. This discovery, like that of Mendelian heredity, was, as you know, made long ago by the Frenchman Jordan; but, as in the case of Mendelism, science ignored it and pursued cheerfully its false path till the facts were rediscovered in recent years. All thorough work has led directly to this result: that any species or kind of organism is made up of a very great number of diverse stocks, differing from each other in minute particulars, but the diversities inherited from generation to generation. This result has in recent years dominated all work on the occurrence of variations; on the effects of selection; on the method of evolution. The condition is particularly striking in organisms reproducing from a single parent, so that there is no mixing of stocks; I found it in a high degree in organisms of this sort which I studied. Thus the in usorian Paramecium I found to consist of a large number of such heritably diverse stocks, each stock showing within itself many variations that are not heritable.[2] *Difflugia corona*, which I have recently been studying, shows the same condition in a marked degree.[3] As you know, a host of workers have found similar conditions in all sorts of organisms. It led to the

[2] See JENNINGS, 1908, 1909, 1910, 1911. (See Bibliography.)

[3] JENNINGS, 1916. (See Bibliography.)

idea of the genotype (Johannsen), as the permanent germinal constitution of any given individual; it supported powerfully the conception of Mendelism as merely the working out of recombinations of mosaic-like parts of these permanent genotypes. The whole conception is in its essential nature static; alteration does not fit into the scheme.

This discovery seemed to explain fully all the observed effects of selection within a species; but gave them a significance quite the reverse of what they had been supposed to have. It seemed to account for practically all the supposed variations that had been observed; they were not variations at all, in the sense of steps in evolution; they were mere instances of the static condition of diversity that everywhere prevails. Jordan, the devout original discoverer of this condition of affairs, maintained that it showed that organisms do not really vary; that there is no such process as evolution; and indeed this seems to be the direct logical conclusion to be drawn. In these days of plots and spies, the evolutionists might almost feel that the enemy had crept into their citadel and was blowing it up from within.

Now, this multiplicity of diverse stocks really represents the actual condition of affairs, *so far as it goes*. Persons who are interested in maintaining that evolution *is* occurring, that selection *is* effective, and the like, make a very great mistake in denying the existence of the condition of diversity portrayed by the genotypists. What they must do is to accept that condition as a foundation, then show that it is not final; that it does not proceed to the end; that the diverse existing stocks, while heritably different as the genotypists maintain, may also change and differentiate, in ways not yet detected by their discoverers.

But of course most of the adherents of the "orthodox genotype theory" do not maintain, with their first representative Jordan, that no changes occur; that all is genetically static in organisms. Typically, they admit that *mutations* occur; that the genotype may at rare intervals transform, as a given chemical compound may transform into another and diverse compound. We all know the typical instances: the transforming mutations of Oenothera; the bud variations that show in a sudden change of

color or form in plants; the dropping out of definite Mendelian units in Drosophila and elsewhere; the transformation of particular Mendelian units into some other condition.

So much then may serve as an outline of a prevailing theory; organisms forming a multitude of diverse strains with diverse genotypes; the genotype a mosaic of parts that are recombined in Mendelian inheritance; selection a mere process of isolating and recombining what already exists; large changes occurring at rare intervals, through the dropping out of bits of the mosaic, or through their complete chemical transformation; evolution by saltations.

Certain serious difficulties appear in this view of the matter; I shall mention merely two of them, for their practical results. One is the very existence of the minutely differing strains, which forms one of the main foundations for the genotype theory. How have these arisen? Not by large steps, not by saltations, for the differences between the strains go down to the very limits of detectibility. On the saltation theory, Jordan's view that these things were created separate at the beginning seems the only solution.

Secondly, to many minds there appears to be an equally great difficulty in the origin by saltation of complex adaptive structures, such as the eye. I shall not analyze this difficulty, but merely point to it and to the first one mentioned, as having had the practical effect of keeping many investigators persistently at work looking for something besides saltations as a basis for evolution; looking for hereditary changes that would permit a continuity in transformation. Some have been searching in the complex phenomena of biparental inheritance; here Castle is to be first named, and in a later lecture you will hear of the views to which he has been led. Others, like Prof. H. F. Osborn, have been searching from this point of view the paleontological records. Others of us have taken up the problem in uniparental reproduction; it is here that my own work falls, and of this I will for a moment speak.

Where reproduction is from a single parent we meet the problem of inheritance and variation in its simplest form; for

there is nothing which complicates genetic problems so enormously as does the continual mixing of diverse stocks in biparental inheritance. In uniparental reproduction we have but one genotype to deal with; we can be certain that no hereditary characters are introduced from outside that genotype.

To hope for results on the problem in which we are interested, we must resolve to carry on a sort of second degree research, as it were. That is, we must accept as a foundation the facts before discovered, as to the make-up of the species out of a great number of diverse stocks; as to the usual effects of selection being nothing save the isolation of such preexisting stocks. What we must do is to take a single such stock—choosing an organism that is most favorable for such work—then proceed to a most extensive and intensive study of heredity, of variation, and of the effects of selection for long periods within such a stock.

Such an organism, most favorable from all points of view, I found in the rhizopod *Difflugia corona*. It has numerous distinctive characters, all congenital; all inherited in a high degree; yet varying from parent to offspring also; none of these characters changed by growth or environmental action during the life of the individual.

Long continued work showed that a single strain of this animal, all derived by fission from a single parent, does differentiate gradually, with the passage of generations, into many hereditarily diverse strains. The important facts about the hereditary variations and their appearance are the following:

1. Hereditary variations arose in some few cases by rather large steps or "saltations."

2. But the immense majority of the hereditary variations were minute gradations. Variation is as continuous as can be detected.

3. Hereditary variation occurred in many different ways, in many diverse characters. There was no single line of variation followed exclusively, nor in the overwhelming majority of cases.

4. It gave rise to many diverse combinations of characters: large animals with long spines; small animals with long spines; large animals with short spines; small animals with short spines;

and so on, for other sorts of combinations of other characters. Any set of characters might vary independently of the rest.

5. The hereditary variations which arose were of just such a nature as to produce from a single strain the hereditarily different strains that are found in nature.[4]

I judge that if the intermediate strains were killed, the two most diverse strains found in nature might well be classed as different species, although the question of what a species is must be left to the judgment or fancy of the individual.

Such then were the results of my own studies as to the nature of hereditary variations and how they appear. How do these results compare with those found by other men? If we take a general survey, we find the following main classes of cases:

1. First, we have the mutations of Oenothera and its relatives: large transformations occurring suddenly. Here is evidently one of the most interesting fields of genetics, but I cannot feel, in view of many extraordinary phenomena in this group, that the bearing on the main problems of genetics is yet clear.

2. Second, we have a large miscellaneous collection of mutations observed in various classes of organisms: "bud variations," dropping out of unit factors, and the like—all definite saltations, but not genetically fully analyzed.

3. In Drosophila as studied by Morgan and his associates, we have the largest and most fully analyzed body of facts which we possess with respect to changes in hereditary character in any organism. The changes here are pictured as typical saltations; but of these I shall speak farther.

4. In paleontology, as the results are presented in recent papers by Osborn,[5] the evidence is for evolution by minute, continuous variations which follow a single definite trend.

5. Finally we have the work in biparental inheritance from Castle and his associates:[6] this, as interpreted by Castle, gives evidence for continuous variation, not following a single necessary trend, but guided by external selection.

[4] The full account of this work is given in JENNINGS, 1916. (See Bibliography.)

[5] See OSBORNE, 1912, 1915, 1916. (See Bibliography.)

[6] See CASTLE, 1915 *a*, 1916, 1916 *a*, 1916 *b*, 1917; CASTLE and PHILLIPS, 1914, etc. (See Bibliography.)

Furthermore, we discover in our survey that there are at least two well-marked controversies in flame at the present time:

First, we have the general controversy between, on the one hand, those who are mutationists and adherents of the strict genotype view; on the other hand, those who, like Castle, believe that we observe continuous hereditary variations in the progress of biparental reproduction. The mutationists attempt to show that the apparent gradual modification of characters observed in breeding is in reality a mere working out of Mendelian recombinations. Here we have contributions by Morgan (1916), Pearl (1916, 1917), MacDowell (1916), Hagedoorn (1914), and others on the one hand; while the full brunt of the attack is borne on the other side by Castle.

Second, we have a somewhat less lively controversy between the genotypic mutationists and the paleontological upholders of evolution by continuous variation. Echoes of this we find in recent publications by Osborn and by Morgan.

Now let us look briefly into the points at issue in the controversy between the "genotypic mutationsts" and the upholders of gradual change during biparental inheritance.

Castle finds that in rats he can, by selection, gradually increase or decrease the amount of color in the coat, passing by continuous stages from one extreme to the other. As to this, he holds two main points:

1. The change is an actual change in the hereditary characteristics of the stock; not a mere result of the recombination of Mendelian factors. This is the general and fundamental point at issue.

2. More specifically, he holds it to be an actual change n a single unit factor; this single factor changes its grade in a continuous and quantitative manner.

On the other side, the critics of these views maintain that the changes shown are not actual alterations in the hereditary constitution at all, but are mere results of the recombinations of Mendelian factors. And specifically, they find a complete explanation of such results as those of Castle in the hypothesis of *multiple modifying factors.*

The method in which these modifying factors are conceived to operate is doubtless familiar to you: their application to Castle's work with selection in rats will serve as an example. There is conceived to be a single "main factor" which determines whether the "hooded pattern" shall or shall not be present. In addition to this there are a considerable number of "modifying factors" which, when the "hooded pattern" is present, increase or decrease the extent of pigmentation. When many of the positive factors of this sort are present, the rat's coat has much pigment; when fewer are present the extent of pigment is less, and so on. The process of changing the extent of pigmentation by selection consists, according to this view, merely in making diverse combinations of these factors, by proper crosses.

This same explanation is applied to a great variety of cases. Castle had carried the war into the enemy's country by predicting (or at least suggesting) that the so-called unit characters in Drosophila would be found to be modifiable through selection.[7] Later research by MacDowell (1915), Zeleny and Mattoon (1915), Reeves (1916), Morgan (1917), and Sturtevant (1917) actually verified this prediction; it has indeed been found that the Drosophila mutations can be modified by selection. Again the mutationists counter the blow with their explanation of multiple modifying factors, which are segregated in the process of selection; and they give some real evidence that such is actually the case.

Now, into the merits of that particular question, as to whether the apparent effects of selection are really due to modifying factors in the manner set forth, I do not propose to enter. Castle maintains that they are not, and I doubt not that he will show you reason for that point of view. At this point my own discussion will diverge from what I judge that he will be likely to give. What I am going to do is to abandon the ground that Castle would defend, proceed directly into the territory of the enemy, accept the conditions met there, then see where we come out in relation to the nature of variation, the effects of selection, and the method of evolution.

[7] See CASTLE, 1915, p. 39. (See Bibliography.)

In no other organism have heritable variations been studied so thoroughly as in Drosophila, and no other body of men have been more thoroughgoing upholders of mutationism and of the multiple factor explanation of the effects of selection, than the students of Drosophila—Morgan, Sturtevant, Bridges, Dexter, Muller, MacDowell, and the others. We may therefore turn to the evidence from Drosophila with confidence that it will be presented with fairness to the mutationist point of view. We shall first ask (1) what we learn from the work on Drosophila as to the possibility of finding finely graded variations in a single unit character. Next we shall inquire (2) as to the relation of the assumed modifying factors to changes in hereditary constitution; to the nature of the effects of selection.

1. First, then, what are the facts as to numerous finely graded variations in a single unit factor? Here we have certain remarkable data as to the eye-color of Drosophila; data that are of great interest with relation to the nature of evolutionary change. This fruit fly has normally a red eye. Some years ago a variation occurred by which the eye lost its color, becoming white, a typical mutation. Somewhat later, another variation came, by which the eye color became eosin. By those wonderfully ingenious methods which the advanced state of knowledge of the genetics of Drosophila have made possible, it was determined that the mutations white and eosin are due to changes in a particular part of a particular chromosome, namely, of the so-called X-chromosome, or chromosome I. And further, it was discovered that the two colors are due to different conditions of the same *locus* of the chromosome; in other words, they represent two different variations of the same unit. Moreover, the normal red color represents a third condition of that same unit.

Somewhat later a fourth condition of this same unit was found, giving a color which lies nearer the red, between the red and eosin; this new color was called cherry. So we have four grades or conditions of this single unit character.

And now, with the minute attention paid to the distinction of these grades of eye color, new grades begin to come fast. In the November number of *Genetics*, Hyde (1916), adds two new grades,

one called "blood," near the extreme red end of the series, the other, called "tinged," near the extreme white end; in fact, from the descriptions it requires careful examination to distinguish these two from red and white, respectively. Thus we have now six grades of this unit. And in the same number of the same journal, Safir (1916) adds another intermediate grade, lying between "tinged" and esoin; this he calls "buff." All these seven grades are diverse conditions of the single unit factor, having its locus in a certain definite spot in the X-chromosome. Such diverse conditions of a single actor are known as multiple allelomorphs.

So, up to date we know from the mutationists' own studies of Drosophila that a single unit factor presents seven gradations of color between white and red, each gradation heritable in the usual Mendelian manner. These grades are the following: (1) Red; (2) blood; (3) cherry; (4) eosin; (5) buff; (6) tinged; (7) white.

Three of these grades have been discovered in the ast five months. It would not require a bold prophet to predict that as the years pass we shall come to know more of these gradations, till all detectible differences of shade have been distinguished, and each shown to be inherited as a Mendelian unit. Considering that the work on Drosophila has been going on only about seven or eight years, this is remarkable progress toward a demonstration that a single unit factor can present as many grades as can be distinguished; that the grades may give a pragmatically continuous series. The extreme selectionist asks only a little more than this.

Besides showing that a unit factor may thus exist in numerous minutely differing grades, this case shows that a heritable variation may occur so small as to be barely detectible. Although the variations do not usually occur in this way, the case presents the conditions which would allow of a gradual transition from one extreme to the other, by means of numerous intermediate conditions. In a population in which were occurring such minute changes as are here shown to be possible, we could get by selection such a continuous series of gradations as Castle describes in

his rats. The difference in the two cases is, that in Drosophila variations which are large steps occur as well as do the small ones; and that, according to Castle's conception of the matter, such minute heritable variations occur more frequently in the rat than in Drosophila. But on the showing of the students of Drosophila, there is scarcely any other difference in principle between what happens in Drosophila and what Castle believes to happen in the rat.

2. But as we have seen, the mutationists reject the view that the changes in the coat color of the rat are due to alterations in a single unit factor; they explain this and other cases of the effectiveness of selection on a single character by *multiple modifying factors*. Accepting again their contention, the question is shifted to the nature of such factors. What sort of things are these modifying factors? What is their relation to actual changes in the heritable constitution of organisms?

Our direct experimental knowledge of these "modifying factors" is scanty. What we have comes again mainly from the studies of Drosophila, so that we need not suspect it of being colored in such a way as to favor the selectionist point of view. We find data as to certain known modifying factors by one of the workers on Drosophila, Bridges (1916), in his recent important paper on non-disjunction of the chromosomes. And here we are taken back again to the series of eye colors, and indeed to one particular member of the series, the middle member, called eosin.[8] Bridges tells us that he found a factor whose only effect was to lighten the eosin color in a fly with eosin eyes; this factor indeed nearly or quite turns the eosin eye white. This factor Bridges calls "whiting." Another factor has the effect of lightening the eosin color a little less, giving a sort of cream color; this is called "cream *b*." A third factor dilutes the eosin color not so much; it is called "cream *a*." In addition to these, Bridges tells us that he has discovered *three other* diluters of the eosin color; we will call them the fourth, fifth, and sixth diluters. And finally Bridges tells us of another factor whose only effect

[8] BRIDGES, 1916, p. 148. (See Bibliography.)

is to modify eosin in the direction of a darker color: this factor he calls "dark." None of these factors has any effect save on eosin-eyed flies.

As you see, these things add tremendously to our gradations in eye color. We had already been furnished seven grades, from white to red; now we have seven secondary grades within a single one of these seven primary grades. Our list of gradations of eye color in Drosophila therefore takes now the following form:

Heritable grades of eye color, due to diverse variations of a single unit located in Chromosome I.	Variations that give modifications of the intensity of eosin, but are located in other chromosomes.
1. White	1. Whiting
2. Tinged	2. Cream b
3. Buff	3. Cream a
4. Eosin	4. Fourth diluter
5. Cherry	5. Fifth diluter
6. Blood	6. Sixth diluter
7. Red	7. Dark

Let us hasten to add that these seven new grades are not located in the same unit factor as are the seven primary ones; their loci are in other chromosomes (or possibly in other parts of the same chromosome).

Here again then we have minutely differing conditions of a single shade of color, brought about by seven modifying factors. Bridges makes the following remark concerning them:

> A remarkably close imitation of such a multiple factor case as that of Castle's hooded rats could be concocted with the chief gene eosin for reduced color, and these six diluters which by themselves produce no effect, but which carry the color of eosin through every dilution stage from the dark yellowish pink of the eosin female to a pure white.[9]

Now this is an extremely interesting statement, one that must arouse the keen interest of the student of the method of evolution. In Drosophila we could get the same sort of graded results that Castle does with his rats, only in Drosophila this is by

[9] BRIDGES, 1916, p. 149. (See Bibliography.)

means of multiple modifying factors, whereas Castle believes that in the rat it is by actual alterations of the hereditary constitution!

But what are these modifying factors? And here we come to the astonishing point. *These modifying factors are themselves alterations in the hereditary constitution.* Bridges leaves no doubt upon this point. He lists and describes them specifically as mutations; as actual changes in the hereditary material.

Where then is the difference in principle between the condition in Drosophila and that in the rat? In Drosophila there occur minute changes in the germinal material, such as to give, so far as our present imperfect knowledge goes, seven diverse grades of a color which is itself only one grade of another series of seven known grades. By means of these graded changes one could obtain, by the mutationist's own statement, the continuously graded results which selection actually gives. What more can the selectionist ask?

There are indeed certain differences in detail, in the notions entertained by the different investigators as to exactly *where* the changes occur. Castle believes that in the rat the changes occur all in one unit—in one chromosomal locus—giving a series like the primary series for eye color in Drosophila. The supporters of multiple modifying factors believe, on the other hand—if we are to accept Bridges' account of such factors as typical (and it is the only account we have)—they believe, I say, that these minute changes have occurred in *some other* part of the germinal material. But this difference is one of mere detail; it does not touch the fundamental question.

This fundamental question is as to the occurrence of these minute changes in the hereditary constitution, and as to the possibility of getting therefrom by selection various grades of a given external characteristic. In this, so far as I can see, there is complete agreement.

Now, doubtless, there is a further diversity in the mental processes of the two sets of men, in that the mutationist thinks of all these numerous grades as after all essentially discontinuous, as a series of steps so minute that the difference between one

and the next one is not detectible. His opponent, on the other hand, perhaps thinks of the series as actually continuous. But the difference is not a pragmatical one; when steps become so minute as to be beyond detection, the question whether they exist becomes metaphysical.

To put the case in brief, if the mutationists are to show that the existence of multiple modifying factors has any bearing on the general question of the effectiveness of selection, they must show that such factors are not themselves minute changes in the hereditary constitution. Not only have they made no attempt to do this, but in the only well-examined cases they state squarely that such factors are indeed alterations in the hereditary constitution.

For the inheritance of such factors as Mendelian units, of course absolutely nothing is required save that the location of the change is in a chromosome. No particular degree of magnitude; no unity of any other kind is required.

But there remains one point brought out by the mutationists which is of great importance to the student of the method of evolution. While they must admit, by their own account, that all these grades occur, so that a practically if not actually continuous series can be formed, they of course point out that the changes do not occur in a continuous series. In the eye of Drosophila variation may occur from red to white directly, without any transitional stages; or from any grade to any other; the continuous scale is obtained only by arranging the steps in order. Therefore, it is maintained, evolution may have occurred by such large steps, not by continuous gradations.[10] This is of course a matter deserving of serious consideration. But certain other points must be considered also. First, the very facts known for Drosophila show that there is nothing to prevent a passage from one extreme to the other by minute changes, just as is held to occur by the paleontologists and selectionists, although change by large steps occurs also. Secondly, in such cases as the eye color of Drosophila we are dealing with char-

[10] See particularly the discussion of this point in MORGAN, 1916, p. 7–27. (See Bibliography.)

acters that are already highly developed. We know for example, that this particular character is formed by the cooperation of many separate parts of diverse chromosomes; it is a highly complex product of evolution. Now, we find that one or another of these parts may suddenly cease to perform its function, so that the red color is not completely formed; there is a sudden change in it; or it may disappear entirely. But is this after all strong evidence that in the original production of this complex character with its numerous underlying functional parts, there was the same change by sudden large steps? Indeed, is it not rather true that such destructive changes in a fully formed character could not be expected to throw light on how that character was built up?

I am not unmindful of the fact that there are a few—but only a very few—cases in which there is indication of a positive addition by a definite step, as when the eosin color is produced in white-eyed stock. But here again the underlying apparatus has before had the power to produce eosin and other colors. The white color was due to the temporary suspension of function in parts of the chromosomal apparatus, and it may be doubted whether the restoration of this function throws light on the way the apparatus was first developed.

To sum up, it appears to me that the work on Drosophila is supplying a complete foundation for evolution through selection of minute gradations. The so-called "multiple allelomorphs" show that a single unit factor may thus exist in a great number of grades; the "multiple modifying factors" show that a visible character may be modified in the finest gradations by alterations in diverse parts of the germinal apparatus. The objections raised by the mutationists to gradual change through selection are breaking down as a result of the thoroughness of the mutationists' own studies. We have already gotten completely rid of the notion that the germinal changes consist only in the dropping out of complete units, or that they are bound to occur in large steps. If the recent rate of progress is maintained, when such an organism as Drosophila has been studied for fifty years, instead of eight or nine, there will be no conceivable gradation

of any character that will not have been detected. The only outstanding difficulty is the fact that large changes occur as well as small ones; this seems perhaps due to the fact that we are witnessing the disintegration of highly developed apparatus in place of its building up.

In all this, except the last point, the work on Drosophila is in agreement with my own observation of gradual variation in Difflugia; with Castle's similar results on the rat; and with the conclusions of paleontologists as to the gradual development of the characteristics of organisms in past ages.

But there is one point in the paleontological conclusions, as set forth in the recent papers by Osborn, which is not in agreement with the experimental and observational results on existing organisms; this I wish to notice briefly. Osborn sets forth that in following given stocks from earlier to later ages, characters arise from minutest beginnings, and pass by continuous gradations to the highly developed condition. This seems in agreement with experimental results, as I have tried here to set them forth. Further, according to Osborn, these developing characters do not show random variations in all directions, but follow a definite course, which might seem to have been in some way predetermined. And this is emphasized by the fact that the same sorts of characters (horns, for example) may arise independently, at different ages, in diverse branches of the same stock, and each follow in later ages the same definite course of development.

It would appear therefore from this that there must be some directing tendency, some inner necessity which drives a developing organ to follow a definite course. Evolution is characterized by Orthogenesis, as this phenomenon has sometimes been called.

Now it appears to me that we do not observe this in the present day experimental work; by selection we can move in more than one direction. I do not mean that the possible variations are not limited by the constitution of the varying organism; they certainly are. But there is no indication, so far as I can see, that the variations push in one determinate direction only.

Now, examining the paleontological summaries further as regards this (I refer to Osborn's papers), we find certain points

that appear to modify seriously, if they do not quite nullify, this conclusion that variations follow a determinate course.

First, we do find that diverse courses are followed by given characters, in diverse branches of a given group; this is particularly true of the characters of shape and proportion, which Osborn calls allometrons. I take it from the descriptions that this is likewise true at times for structural and numerical characters.

A second point which Osborn sets forth is deserving of particular attention. He states, in agreement with Waagen, that in any given geologic stratum, we do find, in addition to characteristics that are in the line of determinate descent, other variations from this line, which are of the sort that constitute what we call at the present time varieties; things that are like the diverse races of Difflugia in my own work. But, say Osborn and Waagen, there is a great difference in principle between these and the others, for those which are in the determinate line of progress persist into the next geologic stratum, while the mere varieties do not. The persistent changes were called by Waagen, mutations (in a sense somewhat diverse from that in which the word is used by de Vries).

Osborn expresses the opinion that these "varieties" may be merely non-heritable modifications.[11] But in our present geologic period we find just such diverging forms, in great number, and we find that their peculiarities *are* heritable; this I emphasized in the introductory part of the present discussion. There is then no reason for supposing that these variations were not heritable in earlier geologic periods; there must have been many races heritably diverse, just as there are now; and these are what Waagen called varieties.

Now since this is so, the only difference between Waagen's mutations and his varieties, is that, on looking backward at them, we find that the former persisted and the latter did not. But this tells us nothing whatever about why the latter did not. It is perfectly possible, so far as these facts go, that it was a

[11] OSBORN, 1915, p. 225. (See Bibliography.)

matter of selection by external conditions; many diverse stocks were present, on an equal footing; some were destroyed, others were not.

What ground then is there for saying that the development of given characters followed a definite course, as if predetermined? The conditions described are exactly what we should require to find if in past ages there were many varied stocks, some of which were preserved by the action of natural selection. Looking back over the series from a later age, we are bound of course to find it a continuous development. If the same characteristics were favorable in successive ages—and there is no reason why they should not be so—then the same sorts of variations would be preserved in those successive ages; a line of development once begun would be continued. And if the same sort of characters are favorable ones in different branches of a family, then similar characters may well arise and follow a similar course of development, in the diverse branches, as Osborn states they do. But at the same time many other heritable variations arise, that are not in the line of progress, and hence are not preserved through selection; these are precisely the "varieties" described by the paleontologists; the diverse races that I have described in Difflugia and Paramecium, and that are found to exist in all organisms. The conditions described by the paleontologists support strongly the theory of evolution by gradual change, but I cannot see that they tend to establish the view that variations show a tendency to follow a definite course, as if predetermined. The paleontologists appear rather to report precisely the conditions which we are bound to find if evolution occurs through the guidance of natural selection operating on a great number of diverse variations, the typical Darwinian scheme.

There is one other point which I wish I had time to take up, but I have not. I will merely attempt to state in a few words my impression of it. This is the point made by Bateson (1914) in his Presidential Address before the British Association, and farther developed by Davenport (1916) in a recent paper: the proposition, namely, that since practically all observed variations are cases of loss and disintegration, we are driven to suppose

that evolution has occurred by loss and disintegration. Davenport combines this idea with the theory that these disintegrating variations follow a definite course, predetermined in large measure by the constitution of the disintegrating material.

There are two points worth consideration in dealing with this theory. The first is one of fact; although it is true that many of the so-called mutations appear to be cases of loss and disintegration, yet there is no indication that this is the case in such effects of selection as have been described by Castle and myself; variations are not limited to any particular direction. Secondly, it appears to me that this conclusion—that because the variations we see are cases of loss and disintegration, therefore evolution must have occurred by loss and disintegration—it appears to me, I say, that this conclusion involves an error in logic, which makes it unworthy of serious consideration. The syllogism which it involves seems something as follows:

1. *Major premise.* Evolution has occurred by progress from the visibly less differentiated in structure to the visibly more differentiated in structure.

2. *Minor premise.* By observation we detect only the visibly less differentiated arising from the visibly more differentiated; we see only a process of decreasing the visible differentiation.

3. *Conclusion.* The visibly more differentiated must have arisen from the visibly less differentiated, by decrease in the visible differentiation of the latter.

The conclusion is absurd; it cannot be drawn save for the fact that while in the two premises we are talking of *visible* differentiation and disintegration, in the conclusion the ground is shifted to mean something entirely different—a sort of inner, invisible, purely theoretical kind of differentiation and simplicity and disintegration. By putting in the word *visible* all the way through, the absurdity is brought to light. All that we can legitimately conclude from the two premises is that we have not seen the process of evolution occurring. If we have seen nothing but loss and disintegration, this is indeed the conclusion that we must draw. But I believe that we cannot assert that this is all that we have seen.

To summarize then what I have obtained from experimental work combined with a survey of the work of others, the impression left is as follows:

1. Experimental and observational study reveals that organisms are composed of great numbers of diverse stocks differing heritably by minute degrees.

2. Sufficiently thorough study shows that minute heritable variations—so minute as to represent practically continuous gradations—occur in many organisms; some reproducing from a single parent others by biparental reproduction.

3. The same thing is reported from paleontological studies.

4. On careful examination we find even that the same thing is revealed by such mutationist work as that on Drosophila; single characters exist in so many grades due to minute alterations in the hereditary constitution as to form a practically continuous series.

5. It is *not* established that heritable changes must be sudden large steps; while these may occur, minute heritable changes are more frequent.

6. It is *not* established that heritable variations follow a definite course as if predetermined; they occur in many directions.

7. It is not established that all heritable changes are by disintegration; although many such do occur, they cannot be considered steps in progressive evolution from the visibly less complex to the visibly more complex.

Evolution according to the typical Darwinian scheme, through the occurrence of many small variations and their guidance by natural selection, is perfectly consistent with what experimental and paleontological studies show us; to me it appears more consistent with the data than does any other theory.

BIBLIOGRAPHY

BATESON, W., 1914. *Address of the President of the British Association for the Advancement of Science.* Science, **40**: 319–333. BRIDGES, C. B., 1916. *Non-disjunction as proof of the chromosome theory of heredity.* Genetics, **1**: 1–52; 107–163. CASTLE, W. E., 1915. *Mr. Muller on the constancy of Mendelian characters.* Amer. Nat., **49**: 37–42. CASTLE, W. E., 1915 *a*. *Some experiments in mass selection.* Amer. Nat., **49**: 713–726. CASTLE, W. E., 1916. *Can selection cause genetic change?* Amer. Nat., **50**: 248–256. CASTLE, W. E., 1916 *a*. *Further studies of piebald rats and selection, with observations on gametic coupling.*

Carnegie Inst. Wash., Pub. 241, Part III: 161–192. CASTLE, W. E., 1916 *b*. *Genetics and Eugenics*. Cambridge. Pp. 353. CASTLE, W. E., 1917. *Piebald rats and multiple factors*. Amer. Nat., **51**: 102–114. CASTLE, W. E., and PHILLIPS, J. C., 1914. *Piebald rats and selection. An experimental test of the effectiveness of selection and of the theory of gametic purity in Mendelian crosses*. Carnegie Inst. Wash., Publ. 195. Pp. 56. DAVENPORT, C. B., 1916. *The form of evolutionary theory that modern genetical research seems to favor*. Amer. Nat., **50**: 449–465. HAGEDOORN, A. L. and Mrs. A. C., 1914. *Studies on variation and selection*. Zeitschr. f. ind. Abst. u. Vererb., **11**: 145–183. HAGEDOORN, A. L., and Mrs. A. C., 1917. *New light on blending and Mendelian inheritance*. Amer. Nat., **51**: 189–192. HYDE, R. R., 1916. *Two new members of a sex-linked multiple (sextuple) allelomorph system*. Genetics, **1**: 535–580. JENNINGS, H. S., 1908. *Heredity, variation and evolution in Protozoa. II. Heredity and variation of size and form in Paramecium, with studies of growth, environmental action and selection*. Proc. Amer. Philos. Soc., **47**: 393–546. JENNINGS, H. S., 1909. *Heredity and variation in the simplest organisms*. Amer. Nat., **43**: 321–337. JENNINGS, H. S., 1910. *Experimental evidence on the effectiveness of selection*. Amer. Nat., **44**: 136–145. JENNINGS, H. S., 1911. *Pure lines in the study of genetics in lower organisms*. Amer. Nat., **45**: 79–89. JENNINGS, H. S., 1916. *Heredity, variation and the results of selection in the uniparental reproduction of Difflugia corona*. Genetics, **1**: 407–534. MACDOWELL, E. C., 1915. *Bristle inheritance in Drosophila*. Journ. Exper. Zool., **19**: 61–97. 1916. MACDOWELL, E. C., *Piebald rats and multiple factors*. Amer. Nat., **50**: 719–742. MORGAN, T. H., 1916. *A critique of the theory of evolution*. Pp. 197. Princeton Univ. Press. MORGAN, T. H., 1917. *An examination of the so-called process of contamination of genes*. Anat. Record, **11**: 503–504. OSBORN, H. F., 1912. *The continuous origin of certain unit characters as observed by a palaeontologist*. Amer. Nat., **46**: 185–206, 249–278. OSBORNE, H. F., 1915. *Origin of single characters as observed in fossil and living animals and plants*. Amer. Nat., **49**: 193–239. OSBORN, H. F., 1916. *Origin and evolution of life upon the earth*. Scientific Monthly, **3**: 5–22; 170–190; 289–307; 313–334; 502–513; 601–614. PEARL, RAYMOND, 1915. *Seventeen years selection of a character showing six-linked Mendelian inheritance*. Amer. Nat., **49**: 595–608. PEARL, RAYMOND, 1916. *Fecundity in the domestic fowl and the selection problem*. Amer. Nat., **50**: 89–105. PEARL, RAYMOND, 1917. *The selection problem*. Amer. Nat., **51**: 65–91. REEVES, EDNA M., 1916. *The inheritance of extra bristles in Drosophila melanogaster Meig*. Univ. Calif. Pub. Zool., **13**: 495–515. SAFIR, S. R., 1916. *Buff, a new allelomorph of white eye color in Drosophila*. Genetics, **1**: 584–590. STURTEVANT, A. H., 1917. *An analysis of the effect of selection on bristle number in a mutant race of Drosophila*. Anat. Record, **11**: 504. ZELENY, C. and MATTOON, E. W., 1915. *The effect of selection upon the "bar-eyes" mutant of Drosophila*. Journ. Exper. Zool., **19**: 515–529.

ETHNOLOGY.—*The Chitimacha of Bayou La Fourche, Louisiana*. DAVID I. BUSHNELL, JR. Bureau of American Ethnology.

Early in the eighteenth century, when the Chitimacha first became clearly known to the European colonists, they occupied two groups of settlements within their rather small territory just west of the mouth of the Mississippi. One group was in the vicinity of Bayou Teche and Grand Lake, on and near the site of the present town of Charenton, St. Mary parish, Louisiana, where some continue to dwell. The second group was

eastward, in the country adjacent to Bayou La Fourche, then known as the river of the Chitimacha. Many, if not all, of the latter removed from their ancient habitat during the years 1718 and 1719, but it is quite evident that other Indians soon occupied the old Chitimacha sites, and among the new arrivals were many Houma, and others from east of the Mississippi. Likewise, it is highly probable that some of the Chitimacha later found their way back to their old homes.[1]

At the present time several families living in Terrebonne and La Fourche parishes, near Bayou La Fourche, claim to be of Chitimacha descent, although they know some of their ancestors to have been Houma, and many have traces of European blood as well. On the following pages are given some of the mannerisms and customs of these people, as related by Abel Billiot, a man about sixty-five years of age, who is known as a Chitimacha, from the village of Point-au-chien in the southeastern part of Terrebonne parish, Louisiana.

HABITATIONS

The primitive form of habitation was constructed and occupied during the past ten years and, according to my informant, the last example was destroyed at the time of the great storm about eight years ago. The houses were built in the following manner: A slight excavation was made to correspond with the floor-space of the future structure. The wall was outlined by a row of posts about three of four inches in diameter and four feet or more in height, placed about two feet apart. Smaller flexible branches and saplings were interwoven between the upright posts. A low conical roof was formed of a frame covered with thick palmetto thatch. A hole was dug in which a quantity of clay and Spanish moss was mixed with water, this serving as plaster for the wall of wattle work. Clam and oyster shells were burned and the lime thus obtained was mixed with water and used to whitewash the inside wall of the

[1] Swanton has discussed this in *Indian tribes of the lower Mississippi Valley*, Bulletin 43, Bureau of American Ethnology, Washington, 1911.

house. On the day this work was to be done all the people of the village gathered, and the walls were completed between sunrise and sunset.[2] The structures were either round or square, and one, much larger than the others, is remembered to have served as a meeting place for the men of the village. The fires were made outside the house. Small shelters are said to have been made of skins.

FOOD

Much food was smoked and so preserved for future use. The method of smoking was this: Four poles were set up, extending between three and four feet above the surface. These were connected at the top by other poles or cords, and to the latter were attached the pieces of meat, the fish, the ducks, and all else that was to be smoked. The fire was made on the ground between the four stakes and no particular kind of wood was required. Many ducks were prepared in this manner, being first skinned, thoroughly cleaned and opened on the back. Everything was so well smoked as not to require more cooking before being eaten.

A hole was made in the ground (probably about the size of a bushel basket) and filled with clams or oysters. These were covered with a thin layer of sand or earth and a fire kindled above. After a certain time the covering was scraped away and the clams and oysters removed and eaten. Neither clams nor oysters are known to have been smoked, they being always plentiful and easily obtained.

Corn was grown and was prepared in various ways. Often it was pounded in a wooden mortar, then passed through a sieve to separate the fine from the coarse particles. The fine meal was mixed with water and allowed to remain over night during which time it would ferment and become sour. The following morning it would be boiled a little, and dried until "it would hold together," then it was eaten. This appears to

[2] This at once suggests a statement made by Adair in reference to the houses of the southern Indians: In one day, they build, daub with their tough mortar mixed with dry grass, and thoroughly finish, a good commodious house.

have been a favorite method of preparing the meal, the sour taste being relished. This was sometimes eaten with a sirup. Again, corn was allowed to ripen on the cob, and to become quite dry, before being shelled. Later the grains were parched by being mixed with hot ashes of a wood fire. The parched grains were then pounded in a wooden mortar, passed through a sieve, and eaten mixed with water.

Turtles were relished, and likewise their eggs, but the flesh was never smoked. Alligator eggs were eaten, but not the flesh unless other food was scarce. Salt was not used by the old people, and it is not cared for at the present time.

Berries and wild fruits are eaten when ripe, but are never dried.

DRESS AND PERSONAL DECORATION

The men, until quite recently, wore their hair long but did not plait it. The manner of cutting and shaping it is not remembered.

Formerly the hands, neck, and cheeks, were tattooed, the colors used being red and black. The former was derived from the juice of the giant rag weed (*Ambrosia trifida*); the latter was soot produced by burning yellow pine.

Small shells were perforated and strung as necklaces. Other shells were worked and pieces were fashioned into beads.

ARTS AND INDUSTRIES

Bowls, spoons, and paddles for stirring food were made of wood. The Chitimacha word for bowl is *Gah-mail'*. Mortars and pestles were made for use in preparing corn, and so forth.

A piece of hard stone is used with a steel to "strike fire," decayed dry wood, or a bit of old hide, being used to ignite with the spark.

The ancient, and still practiced, method of dressing skins as described by Abel Billiot is this: If possible, the skin as soon as removed, is stretched in a frame, or fastened to a flat surface, and in this position is allowed to dry. A frame is then constructed

of four poles, two being set in the ground, parallel and some three or four feet apart, the others being arranged in a horizontal position attached to the former. Within the rectangular space thus formed the skin is stretched and securily fastened. The frame holding the skin is not stood in a vertical position, but at an angle of about thirty degrees. A small fire of palmetto wood is made on the ground below it so that much smoke is produced. Some of the brain of the animal is then rubbed over the upper surface of the skin, which is thoroughly scraped with a chisel-shaped implement made of a piece of hard wood beveled at one end. After this surface has been sufficiently scraped the skin is turned over and the other side is treated with brains and rubbed and scraped in the same manner. During the entire process the skin is kept quite warm by the fire beneath it, and smoke is allowed to rise against it. After the scraping and rubbing, which has resulted in the removal of all hair and wool from one side and all particles of flesh from the other, the skin is quite soft and white. Sufficient oil, from the brain which has been rubbed into the skin, remains to keep it pliable. No other method of dressing skins is known to this man.[3]

Pottery vessels have been made and used within recent years, and are remembered by my informant. A blue clay found near his village was used for the purpose. Mixed with it naturally, was a sufficient quantity of sand, and neither sand nor pulverized shell was added. The clay having been obtained was worked in a wooden mortar, water being added to make a pasty mass. When the clay was of a proper consistency it was modelled into the desired vessel, which was then put in a shady place and allowed to remain two or three weeks before being baked. Tobacco pipes were, and are now, made in this manner. Before being placed in the fire, and after having become thoroughly dry, the vessel was smoothed by scraping with a shell or thin

[3] This method of preparing skins is more suggestive of the north than of the south, and differs greatly from the custom of the Choctaw who live just north of Lake Pontchartrain in St. Tammany parish. BUSHNELL, *The Choctaw of Bayou Lacomb, St. Tammany Parish, Louisiana*. Bulletin 48, Bureau of American Ethnology, Washington, pp. 11–12, 1909.

piece of hard wood, and any incised decorations were added at this time. Brown clay was not considered good for pottery making, and grease was never used to darken the ware. Spoons are made from cow horns.

Baskets are made from both split cane and the stems of palmetto, native dyes being used. Three colors are made and used by these people, yellow, red, and black, these added to the natural material giving them four shades to combine in their basket work. The yellow dye is derived from the roots of l'anze, which is known locally as paciance. This is a dock, probably *Rumex crispus* L. The root is crushed and pounded while fresh and is not allowed to become dry. The crushed roots are then placed in a kettle of water and allowed to boil. The split cane is also put in the kettle and soon becomes a brilliant yellow. If a red is desired the material is first dyed yellow, as stated, and is then removed from the kettle and allowed to remain over night in a protected spot, away from the wind. In the morning it is again put in the same liquid to which is added a small quantity of lime made by burning clam or oyster shells. It is again boiled and now turns red, caused by the action of the strong alkali. The shade of red may be governed by the density of the yellow dye, and by the length of time it remains in the alkali solution. Black dye is made by boiling the bark of the maple or live oak in water.

Cords or ropes are still made by twisting back moss. Only *black* moss will serve the purpose. A quantity is obtained and two persons work with it, twisting in opposite directions. After the cord is some twenty or thirty feet in length it is doubled and again twisted, this time very tightly.

Dugouts or pirogues are made of a single log of cypress. Other woods are sometimes used, but cypress is the favorite. Light canoes were formerly made by covering a frame of wood with skins or bark.

HUNTING AND FISHING

Blowguns were used in hunting small game and birds. They were made of pieces of cane from 5 to 7 feet in length.

Formerly the cane was split, the joints hollowed out and the inside smoothed, then the two pieces were bound together. More recently a piece of wire has been used to burn the joints. The darts were wrapped for a distance of about two inches with cotton or frayed cloth. Bows and arrows were used by the "old people," but not since they have been able to obtain guns and powder from the whites. Fish are secured by spearing them from a dugout, usually when the sun is shining brightly. Traps for catching fish are unknown to my informant, but nets are known to have been used.

Little could be learned respecting the social culture of these people, the native names of people and places having been forgotten. The myths and legends are no longer remembered, although several very old persons, now living east of the bayou near the coast, may retain some knowledge of their ancient tribal organization. All will soon be lost.

ABSTRACTS

Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. Each of the scientific bureaus in Washington has a representative authorized to forward such material to this journal and abstracts of official publications should be transmitted through the representative of the bureau in which they originate. The abstracts should conform in length and general style to those appearing in this issue.

GEOLOGY.—*The Cleveland Gas Field, Cuyahoga County, Ohio, with a study of rock pressure.* G. SHERBURNE ROGERS. U. S. Geological Survey Bulletin 661–A. Pp. 68, with 2 plates and 13 figures. 1917.

Most of the wells in the Cleveland gas field, which is located in the westerly suburbs of the City of Cleveland, are from 2500 to 2900 feet deep and draw their supplies from the so-called Clinton sand. The structure of the region is, broadly speaking, monoclinal, the formations outcropping in the western part of Ohio and dipping to the east beneath the great Appalachian Coal Basin. The Clinton sand itself does not outcrop, however, but feathers out in the central part of the state, and the Cleveland field and other important oil and gas fields to the south occupy a belt along the upper or thinning edge of the sand. The accumulation of the gas at Cleveland has been influenced also by structural considerations, the field being roughly coincident with a broad gentle bulge or nose on the monoclinal slope, and has been affected locally by the porosity or texture of the sand.

Owing to the very rapid growth of the field which was started in 1912, and which in 1915 contributed about 40 per cent of the Ohio production, the records of pressure and production decline are unusually complete and afford an interesting study. It appears that in any large group of wells there is a fairly close average relation between rock pressure and production. Hence by studying the pressure decline in any group it is possible to predict in advance of drilling the probable flow or volume of a new well in that group. If desired, the estimate may be evaluated according to the theory of probability and the number of chances in a hundred that the flow will not exceed a given figure may be calculated. Although the actual figures cited of course hold only in the Cleveland field the principles discussed should be applicable in many areas. G. S. R.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

The 316th meeting was held at the Cosmos Club, February 28, 1917.

INFORMAL COMMUNICATIONS

FRANK J. KATZ: *Age of the Worcester phyllite.* (No abstract.)

REGULAR PROGRAM

R. V. A. MILLS and R. C. WELLS: *The evaporation of water at depth by natural gases.* During the production of petroleum and natural gas in the Appalachian fields the associated waters undergo a definite order of change in the proportions of their dissolved constituents. Along with other gases, carbon dioxide escapes from solution while calcium carbonate and ferrous carbonate are deposited in the wells and in the interstices of the oil and gas bearing rocks. Concentration is brought about by the removal of water as moisture in the escaping gases and at the same time salt is lost from solution. The composition of the salt deposit is approximately: NaCl 94 to 99.5 per cent, $MgCl_2$ 2.5 to 0 per cent, $CaCl_2$ 3.0 to 0 per cent, and $CaCO_3$ 0.5 to 0 per cent. The composition of this salt was found by analyses of salt deposits collected from oil and gas wells and gas line pressure regulators; and this composition agrees closely with the composition of the salt deposit that can be predicated from the changes in the composition of the waters during their concentration under known conditions of production. Some of the pressure regulators, in which gas pressures are reduced from line pressures to distributing pressures, are several miles distant from the wells from which gas is derived. Analyses of salt deposited in the wells and regulators bear a striking resemblance to analyses of salt from Louisiana salt domes and salt beds in New York, Michigan, and Kansas.

Comparisons of analyses of Appalachian oil-field waters from different stratigraphic horizons in the same localities indicate that salt of the same general composition has been lost from solution during natural concentration. The investigators are convinced that the evaporative action of natural gas has been an important geologic factor in the changes which many deep-seated waters have undergone. During geologic time enormous volumes of natural gas have passed

through the sediments, so that sea water, which was interstitially included in marine sediments, has probably undergone a like mode of concentration.

Judging from experimental results obtained with illuminating gas, methane should be no exception among gases in its capacity to take up and transmit moisture. The amount of moisture in a given volume of gas under any given conditions can be calculated from the vapor pressure of solutions of salt in water. Conditions favorable for the transmission of water as vapor in porous rocks may be created by the chemical production of gases, by the expansion of compressed gases, by the release of gases held in solution in oil or water by pressure, and by the vaporization of liquefied gases.

Salt and brines are commonly found near occurrences of gas and petroleum, so that gases may have been effective in the evaporation of the solutions which yielded the salt. In fact, salt domes may be the result of evaporation and crystallization brought about by the passage of expanding natural gas which has carried off moisture as vapor and possibly also has cooled the solutions from which salt was crystallized.

The great changes effected in the waters of the Appalachian fields during the production of oil and gas were explained in part by the mode of occurrence of oil-gas-water mixtures in this region of low dips. The occurrence of gas and oil overlying water was attributed in part to induced segregation brought about during movements through the pay sands after production was begun.

M. I. GOLDMAN: *Results of the microscopic examination of some rocks from the oil fields of southeastern Ohio.* This is a preliminary study and the conclusions are therefore only tentative. The primary object of the investigation was to find any characters of the rocks that might bear on the distribution of oil in them. As the work progressed it appeared that, in addition, light might be thrown on some of the early stages of metamorphism and their relation to geologic conditions It was found impracticable to make determinations of pore space because, by the tearing out of soft minerals, and in other ways, holes are produced in grinding the thin section. The investigation was therefore limited to the study of epigenetic minerals. The following were recognized: *Quartz* as a secondary growth around the original grains and in continuous crystallographic orientation with them, was found in almost all not too argillaceous sandstones. The approach to quartzite was in general greatest in the deepest beds, though this factor was very variable. *Kaolinite* in fine crystalline aggregates filling the pore spaces. This is known as a common mineral in sandstones. *Calcite* and other carbonates. These seemed to be particularly abundant in older fields or fields in which wells were very numerous, thus indicating a relation to recent circulation of ground, waters resulting from the drilling. *Sulphides* (pyrite or marcasite) were scarce in this field and occurred mainly in the clays associated with carbonaceous matter, a common syngenetic relationship. It is significant, however, as illustrating the mineralogic

and chemical individuality of different fields that, outside of this field, in Ohio and Pennsylvania they are sometimes common in large aggregations in sandstones. *Chert* and *opal* growths are scarce and difficult to recognize, and received no special attention. *Micas* are probably the most significant of the secondary minerals. They are of various species and types and their differentiation from syngenetic micas is difficult, being based mainly on habit and distribution. These most require further study, but so far as can at present be stated their development seems to be, like that of the carbonates, related to circulation resulting from opening up of wells. There were some indications that the development of secondary quartz and sulphides might also be related to this feature.

A final interesting metamorphic character is incipient granulation, apparently by pressure and followed by recementing of sand grains in a few of the deepest beds, especially in one sample at a depth of 2230 feet.

Of the relation of oil to these factors nothing can as yet be said. The most porous, which are generally the purest and coarsest sandstones, naturally form the best reservoirs, but they are often barren. A dark brown stain, believed to be due to oil, is also found related to the occurrence of carbonaceous matter in some of the clays, a relationship well recognized in the oil shales of the west.

The 317th meeting was held at the Cosmos Club, March 14, 1917.

REGULAR PROGRAM

HARRY FIELDING REID: *The distribution of land and water on the earth.* (No abstract.) Discussed by EAKIN, UMPLEBY, and W. J. HUMPHREYS.

WILLIAM BOWIE: *Some evidences of isostasy.* The first exhaustive test of the theory of isostasy was made by Prof. John F. Hayford, about ten years ago, while he was in charge of the geodetic work of the United States Coast and Geodetic Survey. For this purpose he used the connected triangulation of the United States and the astronomic observations made at several hundred triangulation stations. His results were surprisingly convincing that isostasy existed to a remarkable degree.

The speaker reviewed briefly the evidences of isostasy, as given by the most recent investigations of the subject by the Coast and Geodetic Survey. In these investigations there were used 219 gravity stations in the United States, 42 in Canada, 73 in India, and 40 other stations, which were principally in Europe. The conclusion reached by the speaker was that isostasy, for areas as large as the United States, is perfect and that it is practically perfect for areas of a much smaller size; such, for instance, as the state of Texas, or an area even smaller.

It is impossible to tell, with our present data, whether the local deviations from normal gravity, called anomalies, are due to a lack of

isostatic compensation locally or whether they are due to abnormally heavy or light materials near the station but in the region below sea level. This local deviation from normal in the gravity may be due in part to an actual deviation from perfect isostasy and in part to the abnormal densities near the surface.

It is reasonably certain that the anomalies cannot be due entirely to the use of erroneous densities for the topography, that is, the material which is above sea level, nor to the method of distributing the compensation with respect to depth. The largest anomaly, which is 0.093 dyne, is at Seattle and this is the equivalent of 3000 feet of material of normal density. As a matter of fact, Seattle is located near the coast and that station has an elevation of only 74 meters. We must, therefore, conclude that the anomaly at this station is caused by conditions which exist below sea level.

It has been found, as a result of the investigations, that a regional distribution of compensation out to a distance of 58.8 kilometers from a station is just as probable as the local distribution of the compensation. It has also been found that a regional distribution out to a distance of about 167 kilometers from the station is not so probable as the regional distribution to the smaller distance mentioned above or as a local distribution. There is no relation between the gravity anomalies and the character of the topography. There is, however, a decided relation between the gravity anomalies and certain geological formations. There is a very strong tendency for gravity to be light and the anomalies negative for stations located on the Cenozoic formation and the opposite is true for stations on the pre-Cambrian formations.

If the compensation were assumed to be distributed uniformly with respect to depth, then the depth of compensation, as derived from gravity data alone, is 95 kilometers. This agrees fairly well with the depth obtained from deflections of the vertical, but there is no geodetic evidence that the compensation is distributed uniformly with respect to depth. It is possible that some other method of distribution is the true one but it is difficult or practically impossible ever to discover, at least from geodetic evidence, what the true method of distribution is.

In the other countries data as detailed as for the United States were not available, but such tests as could be made indicated that isostasy in them was about as perfect as in this country. It is hoped that geodesists in the other countries will reduce their gravity stations for topography and isostasy by the same method as that used by the United States Coast and Geodetic Survey or by a similar one in order that more exhaustive tests may be made of the isostatic condition throughout the world.

Discussed by WHITE.

DAVID WHITE: *Discussion of gravity anomalies from the stratigraphic standpoint.* (No abstract.) Discussed by WILLIAM BOWIE.

H. E. MERWIN, *Secretary.*

THE BOTANICAL SOCIETY OF WASHINGTON

The 119th regular meeting of the Society was held in the Crystal Dining Room of the Ebbitt Hotel, Wednesday evening, March 14, 1917. Seventy-four members and sixty-five guests were present. Mr. T. H. KEARNEY, President of the Society, presided. Mr. A. S. HITCHCOCK, the retiring President, delivered an address on *Taxonomic botany and the Washington botanist.* A full text of this address appears elsewhere in this Journal.[1]

The 120th regular meeting of the Society was held in the Assembly Hall of the Cosmos Club at 8.00 p.m., Tuesday, April 3, 1917; eighteen members and one guest present.

Under "Brief Notes" Dr. W. H. EVANS showed a Lummière plate of a pineapple field in Hawaii, part of which had been sprayed with an 8 per cent solution of ferrous sulphate. The sprayed portion of the field was a deep green color and produced an excellent crop while the unsprayed portion was yellow or pale green and the crop practically a failure.

The regular program consisted of the following papers:

The control of the white pine blister rust: HAVEN METCALF. (Illustrated with lantern slides.) The white pine blister rust apparently originated in Asia and spread into Europe upon *Pinus cembra*. When the white pine (*Pinus strobus*) was introduced into Europe it proved to be subject to the disease. The first record of importation of white pine transplants from Europe to the United States dates back only to 1899. The disease was first reported in America in 1906. Since that date enormous quantities of diseased nursery stock have been imported. The pathologists and foresters of New England and the Middle Atlantic States discouraged importation of white pine seedlings after June, 1909, and importations was made illegal in 1912.

The disease has spread more rapidly and posessed greater virulence in New England than it did in Europe. At first it was hoped the disease would prove to be only a disease of nursery stock but at several points in New England it is attacking large trees. Inspection of nursery stock for blister rust is largely futile since the rust often incubates in pine tissue for many years before becoming apparent by distorting the growth or by fruiting. According to Ravn this incubation period may be as long as 20 years. As alternate hosts the black currants, both wild and cultivated, are particularly subject to the disease, and in areas of general infection are reliable indicators of its presence. On Ribes the disease is generally prevalent throughout New England, which means that the actual infection of pine is much more general than is shown at present.

[1] 7: 251. 1917.

The control of the disease in America presents three separate problems: (1) In the territory west of the Mississippi river the disease is not known to occur. During the coming season an extensive survey will be made of these states to determine whether the disease is or is not present. If it has not been carried into this territory on nursery stock, there is little possibility of its getting in by natural means. If the disease should once become established under western forest conditions, its control would be hopeless. All 5-needle pines of this area, including the very valuable sugar pine and western white pine, are subject to the disease, and many species of wild Ribes are common. (2) In the states between the Mississippi and the Hudson River, there is an area about 30 miles square in Minnesota and Wisconsin, northeast of St. Paul, which is heavily infected. In Indiana, Ohio, Pennsylvania, and New Jersey the disease has been found in a few nurseries and plantations, and is now believed to have been eradicated. In New York it has been found in both nurseries and plantations, and largely eradicated. (3) In New England the infection is so general that the only hope of successful growing of white pine in the future lies in the elimination of the alternate host of the disease. Whether such elimination can be made at a sufficiently low cost to be profitable has not been determined. Probably in localities where Ribes occur sparsely, as in Connecticut and Rhode Island, a great deal can be accomplished.

Dr. Metcalf called attention to the larger problem of free trade in plant diseases and in insect pests. He questioned it as a sound national policy and doubted if the entire *importing* nursery business is worth as much to the country as the damage which it causes. Not a single plant disease or insect pest that has once become established in this country has been eradicated or is ever likely to be. No matter how well controlled, it remains in every case a permanent tax against our economic resources. To safeguard the country against further invasions is therefore most important.

Technique for the study of the white pine blister rust: R. H. COLLEY. (Illustrated with lantern slides.) The study of the blister rust requires a knowledge of the anatomy of the hosts and of the life history and morphology of the fungus itself, since cultural studies are necessarily confined to the host plants. The best available methods of killing, embedding, and sectioning are being employed in working out the relation of host and parasite. Flemming's killing solution followed by Haidenhain's iron alum-haematoxylin and a counter stain of lichtgrün or orange G have proved very satisfactory for both host and parasite in both the pine and the currant stages. For quick sectioning of doubtful specimens previous to diagnosis the freezing microtome has been very satisfactory. Excellent preparations can be cut, stained, and mounted in two hours. The most satisfactory stains seem to be safranin and lichtgrün. When correctly handled, the hyphae and haustoria of the rust are sufficiently characteristic to be of positive diagnostic value, even before spores are formed.

Some natural groups in aspergillus: CHARLES THOM. (Illustrated with cultures.) A large number of strains obtained in cultures of Aspergillus fall into a series of natural groups. In these groups certain general morphological and physiological characters prevail. The differences between the members of the groups are for the most part quantitative rather than qualitative. In the *Aspergillus niger* group, the amount or intensity of color varies markedly among the individuals, other characters remaining the same. Individuals with exactly the same color and morphology show markedly different physiological activities. The differences between these latter physiological forms is certainly as important as the mere difference in the amount of color secreted in the spore, which difference has been commonly accepted as a basis for specific description.

Another great group may be made of the forms giving the morphology of *Aspergillus flavus*. In this series again the same condition exists as noted in the *Aspergillus niger* group. *Aspergillus fumigatus* and *A. nidulans* form two closely related series of organisms.

Several similar series may be made but the point clearly brought out is that there is evidence of mutation among these forms in which a single morphological or physiological character appears to separate one strain from the typical strain of the series as at present described. Within each group differences are easily recognizable, some of which are determinable only by chemical or physiological means. The solution of the taxonomic difficulty offered is that each group shall be designated by the name of one of its well-known species excepting when particular strains have become important economically or physiologically, and must therefore receive some further form of identification and description.

H. L. SHANTZ, *Corresponding Secretary.*

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 569th regular meeting of the Society was held in the Assembly Hall of the Cosmos Club, Saturday, April 7, 1917; called to order at 8 p.m. by President HAY; 45 persons in attendance.

Under the heading "Brief notes and exhibition of specimens" Dr. R. W. SHUFELDT exhibited lantern slides of living California quail, calling attention to their rapidly diminishing numbers. Dr. L. O. HOWARD called attention to a specimen that had lately come to his notice, the cocoon of a Cecropia moth containing moonstones. He expressed the opinion that they had been placed there by a thieving crow or bluejay. Mr. A. W. WETMORE stated in this connection that he has seen bluejays insert small acorns and kernels of corn into large cocoons.

The regular program consisted of two communications:

ALEXANDER WETMORE and FRANCIS HARPER: *A note on the hibernation of the mud-turtle.* The authors reported finding a specimen of *Kinosternon pennsylvanicum* shortly after it had left its underground

winter-quarters. The hole from which it had emerged was beneath a dense growth of green-briar in an old field, and about 50 yards from the nearest marsh. The burrow was 9½ inches deep, and was open save at the lower end, where the animal had apparently lain encased in a mass of mud. The actions and condition of the turtle after being placed in water were described in detail, and an account of a post-mortem examination of the viscera was given. Messrs. W. P. HAY, M. W. LYON, JR., and WM. PALMER took part in the duscussion.

A. S. HITCHCOCK: *Botanizing in the Hawaiian Islands.* The speaker visited the Hawaiian Islands during five months of 1916. He said the trade winds deposit their moisture upon the eastern and northern mountains of all the islands, furnishing the conditions for rain forests in these regions. The lee side of the islands is dry even to aridity. An interesting feature of the wet areas at or near the summit of the ridges are the open bogs. These bogs are devoid of trees and large shrubs, but contain a variety of low shrubs and herbaceous plants. Many species form tussocks, or hemispherical masses, raised above the level of the bog. The most conspicuous of the tussocks is made by a sedge (*Oreobolus furcatus*, Mann). Three peculiar species of Panicum are tussock-formers (*Panicum monticola* Hillebr., *P. imbricatum* Hillebr., and *P. isachnoides* Munro). Owing to the extreme isolation of the islands, the flora is peculiar and interesting. The family Lobeliaceae is represented by about 100 species, belonging to about 6 genera. Many species are arboreous, forming trunks 10 to 20 feet or in a few cases as much as 40 feet high. The crown of foliage gives the aspect of a palm. The grasses, disregarding the introduced species, are not numerous, but several are peculiar. The genus Eragrostis is represented by numerous species. A rare species of Poa (*Poa siphonoglossa* Hack.) produces leafless rushlike stems, as much as 15 feet long. The talk was illustrated by maps, botanical specimens, and numerous lantern slide views of various features of the islands.

M. W. LYON, JR., *Recording Secretary.*

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